

MEMORANDUM

RECEIVED

TO: CHARLES R. WALL AUG 15 1990

FROM: BERNARD V. O'NEILL, JR. (x2561) CHARLES R. WALL
DATE: AUGUST 14, 1990 PM COMPANIES INC

RE: TWO PROJECTS: LETTERS TO DR. MANUEL AND EYSENCK PROPOSAL

I am returning to you the original materials you sent me on the two projects you asked me to review. In regard to the Eysenck matter, we discussed the termination of Eysenck's research as a CTR Special Project several years ago. Pat Sirridge and I recommend that no further funding be provided. The manuscript by Eysenck which you forwarded titled Smoking, Personality and Stress As Risk Factors for Cancer and Coronary Heart Disease, has been reviewed. It appears to be a review paper updating his text titled The Causes and Effects of Smoking, 1980. The same themes present in that text also appear in this manuscript. Further, the reprint (also enclosed in the material you sent) dated April 1990, titled Personality, Stress and Disease: Description and Validation of a New Inventory echoes the familiar theme that after stress and personality factors are controlled for, there is nothing left for smoking to explain in a statistical sense. In brief, there is nothing remarkable in these items except for updated literature in the draft manuscript. I agree with Mr. Lincoln's suggestion in his memorandum to Mr. Resnick, dated July 16, 1990, last paragraph, that no conclusions different from what has already been published by Dr. Eysenck would result from additional calculations.

In regard to the other project, which included two draft letters prepared for Mr. Resnick's signature and addressed to Dr. Barry Manuel of the Massachusetts Medical Society, it is my recommendation that neither letter be sent. The second draft letter, dated July 18, 1990, consists of two paragraphs, the last of which requests Dr. Manuel to review the materials prepared by Mr. Lincoln as contained in the first letter. This is a most unusual and inappropriate request for Dr. Manuel. In my opinion, it would either be ignored or, and, I think this is possible, though unlikely, given the antismoking climate in Massachusetts, the paper would be subject to a scrutiny which would bring out its weaknesses and could result in unfortunate publicity. It also raises the question of what Philip Morris would be obligated to do once it received a severe critique of a paper prepared by one of its retired

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officials and forwarded by one of its current executives. I can foresee some difficult questions, possibly asked at deposition, about what was done with the response which the company had requested. Also, questions about whether such requests for reviews of company employees' views were ever made before, and with what results could be asked.

In regard to the longer draft letter dated July 9, 1990, I recommend that it not be sent for several reasons. You are undoubtedly aware that Don Hoel is planning, with the assistance of Dr. Helmut Gaisch, a symposium on the question that is at least partially addressed in the letter: the inconsistencies in worldwide statistics between tobacco consumption and death rates from various diseases. This symposium is designed to provide a response to claims based on WHO statistics, arising from the recent conference in Perth, Australia. I mention this because if that conference is successful, the industry would have a current response of some substance to this issue. Second, in my view, the letter's effectiveness is limited because the point of attack it makes appears not to be very strong. I am attaching the relevant pages of the "Morbidity and Mortality Weekly Report," April 6, 1990, for your information. The letter addresses on page 1, the information in Table 1 of the paper, showing that the age adjusted all cause death rate for both sexes in Japan is the lowest among all countries listed. Mr. Lincoln then uses other data, not in the article, in his letter, paragraph 2, page 1, referring to the smoking prevalence among Japanese males. He then attempts to show the inconsistency, under the causal hypothesis, between higher prevalence and lower mortality in Japan. There are no data in the "Morbidity and Mortality Weekly Report" dealing with specific prevalence information in Japan.

The draft letter, paragraph 1, page 2, makes reference to the Keys study. Since the publication, Keys himself has been critical of how the industry has used some of the results from his study. Because of this, and the age of the study, and the fact that it was not referenced in the article in question, I do not believe a reference to Keys makes a good point.

I am very much concerned with paragraphs 2 and 3 on page 2 and paragraph 1 on page 3 because of its reference to private computations made for Mr. Lincoln and apparently never published. A request for these computations, offered by Mr. Lincoln when he writes "details on request," could provide plaintiffs' attorneys with materials possibly embarrassing to the company.

The second paragraph on page 3 makes reference to data on teenage smokers in a study with which Dr. Manuel has no

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connection. The reference to teenage smokers may not be well advised in view of the current campaigns against children smoking that are underway under the direction of The Tobacco Institute. Further, the reference to the fact that the heavier smokers are those with very poor social and family relationships is not a point the company should be anxious to make.

The last paragraph on page 3 and top of page 4 references a study with which Dr. Manuel apparently had no connection. I have reviewed this paper (published in 1990), and found it to be a study of only 173 white males focused mainly on psychosocial health in later midlife. Table 4 in this study (mentioned by Mr. Lincoln on page 4, four lines from the top) listed only the "most robust predictors" taken from two other tables in the paper; pack/years of smoking was not one of the top predictors. In other words, smoking was an independent predictor of health, but was not as robust a predictor, for example, as "ancestral longevity," "childhood strength," and "vigorous exercise in college." Mr. Lincoln suggests that smoking is not an independent predictor when a multivariate analysis is performed and this conclusion is wrong. Further, the point he is trying to make is not particularly strong, because of the small sample size and the weaknesses in determining health status by a five point scale, as explained on page 32, first column in the paper. I have enclosed a copy of this study for your information.

The first full paragraph on page 4 refers to a very old study by Rose and Bell, again its relevance to the article in question is not apparent. The final paragraph on page 4, extending over to page 5, is somewhat unclear to me in that Mr. Lincoln is referring to modeling rates as the "sum of the possible effect of smoking itself and a possible net effect of all the other differences between smokers and nonsmokers." Unless this is made clear, I am not sure how Dr. Manuel would respond to it. There are no published references to this concept in this paragraph. I also think that the tone of this paragraph, especially in the words "abandon all scholarly caution in their zeal to contribute to campaigns against smoking" is shrill and not appropriate in a letter apparently directed to scientific issues. The final paragraph on page 5 contains beliefs about factors affecting mortality and disease with no references. I do not believe it is appropriate in a letter which ostensibly is addressing a scientific article.

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